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"QUEEN ANNE . . ." AND ALL THAT: A RESPONSE

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On pages 377-381 of *JETS* 20/4 (December, 1977) Richard A. Taylor offers a review of my recent book, *The Identity of the New Testament Text.* At the bottom of p. 380 he raises the possibility that I fear misrepresentation. Indeed, on p. 143 of my book I do imply such a fear, and it seems to me that Taylor's review furnishes ample justification for my apprehension. I find it difficult to understand how he could read my book with attention and still express himself as he does. In the interest of truth and fair play I should like to comment on the more salient infelicities in the review.

Most seriously misleading is the representation that I am calling for a return to the Textus Receptus (TR). In the first paragraph of the review we read:

> True progress can be made, Pickering feels, only when scholarship returns to the "majority" Greek text as (usually) represented by the printed TR. Other recent writers have also called for a return to the TR; one thinks immediately of Edward F. Hills, Zane C. Hodges (who wrote the foreword to Pickering's book), T. H. Brown, D. A. Waite, J. J. Ray and David Otis Fuller.

While men like Brown, Fuller and Hills *do* call for a return to the TR as such, Hodges and I do *not.* We are advocating what Kurt Aland has called the majority text.[1] The whole of chap. 7 (in my book) is given to a presentation of how I propose to determine the identity of the text, and on p. 177 I plainly state that the TR will probably require correction in over a thousand places.

In the second paragraph the reviewer welcomes my criticism of the Westcott-Hort (W-H) textual theory, observes that I add little that is new (which is true), lists some inadequacies of the W-H theory, and ends by saying, "Surely Pickering does not think that a relisting of these problems argues *ipso facto* for a return to the TR." Taylor is quite right—I certainly do not think that a mere relisting would argue any such thing, and my book neither says nor implies that I do. Although the weaknesses of the W-H theory have been pointed out by many over the years, I believe my fourth chapter is the most organized and compact (yet reasonably thorough) presentation of them that has appeared. Although scholars like Aland, Colwell and Zuntz have criticized the W-H theory at various points, to my mind they have not accepted the logical consequences of their work. As I point out on p. 166 n. 9, the major

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[1]K. Aland, "The Significance of the Papyri for Progress in New Testament Research," *The Bible in Modern Scholarship* (ed. J. P. Hyatt; New York: Abingdon, 1965) 342.

tenets of the W-H theory are like the floors in a multistoried building: Each level depends on the one below it. If one or more floors are removed one cannot reasonably continue living in the penthouse as if nothing had happened. I have presented evidence to the effect that all the floors below the penthouse, including the foundation, have been removed.

In the fifth paragraph the reviewer opines that I do not really perceive "the *a priori* force of the genealogical argument." Although he admits that "exact genealogies for actual manuscripts cannot be worked out" (with extant materials), he goes on to say:

> But the logical force of the genealogical argument, so far as the TR is concerned, remains: The fact that a text has a *majority* of support does not *necessarily* mean that it is correct. It may or may not be, and to defend the TR simply on the basis of majority is inadequate.

As a theoretical statement I agree that a text's having a majority of support does not necessarily mean that it is correct—so much so that my seventh chapter discusses seven factors (only one of which is "majority") that should be taken into account in determining the text. I do not defend the TR as such at all, and I would not defend any text "simply on the basis of majority." In practice I would insist that "genealogy" may legitimately be used only where there are demonstrated relationships, as in the case of Family 1 or Family 13.

In the sixth paragraph the reviewer discusses deliberate scribal alteration in the text. He is quite unfair when he suggests that "to Mr. Pickering the scribes were all demons. . . ." Perhaps the main point of my fifth chapter is that the large majority of scribes tried to do an honest and careful job. It was a small minority that engaged in deliberate alteration of the text.

In the seventh paragraph Taylor suggests that my thinking is "somewhat clouded" on the subject of Codex D. Whereas I speak of "an inveterate propensity for omission" in Luke 24 (p. 61), he reminds us that D is characterized by additions. That may be so in Acts, but it is not so in the last three chapters of Luke. Just in those chapters D omits 354 words, adds 173, substitutes 146 and transposes 243. Westcott and Hort followed D in omitting 25 words (in those chapters), so in their judgment D omitted 329 words, while adding 173, and so forth. It is a commonplace of textual criticism that the quality of a manuscript may vary considerably from book to book and even from chapter to chapter. For a quick and easy lesson on this subject the reader may consult G. D. Fee.[2]

In the eighth paragraph I am accused of a *non sequitur.* The reviewer notes that I give six pages (pp. 94-100) to a presentation of evidence to the effect that early Christians received the NT writings as authoritative from the start, and he grants the point. But he then states that such an attitude has no necessary bearing on the question of whether

[2]G. D. Fee, *Papyrus Bodmer II* (P^{66})*: Its Textual Relationships and Scribal Characteristics* (Salt Lake City: University of Utah Press, 1968) 12-13.

or not they exercised care in the transmission of the text. Why did Taylor neglect to mention that I devote the next four pages (pp. 100-104) to a discussion of precisely that question? Aside from the evidence adduced that they were watchful and careful in transmitting the text, would not their attitude toward the text enhance their watchfulness and care? Will anyone argue that reverence for the text would cause them to be careless in its transmission? I submit that their attitude toward the text is germane to my contention.

Next the reviewer charges that I use "emotionally-loaded language." Happily, he gives page references for the instances he cites. I invite the reader to evaluate my phraseology in context, taking account of the documentation, and then to form an opinion as to its appropriateness.

As to the several points the reviewer raises on p. 380, if the reader will turn to the appropriate places in my book he will find evidence and documentation. Several points call for clarification, however. We are told that "Pickering . . . asserts that '7Q5, 4, and 8 tend to confirm the history of the text presented in this volume' (p. 148)." What I actually wrote is, "It seems to me that 7Q5, etc." As to the lack of early "Byzantine" MSS, it was Lake, Blake and New who said that "it is hard to resist the conclusion that the scribes usually destroyed their exemplars when they had copied the sacred books."[3] They could think of no other way to account for the fact that in three ancient monastic libraries equipped with scriptoria they found no "parents," only "orphan children." How would the reviewer account for this complete absence of parents? However that may be, I argue that use and climate were the determining factors (pp. 123-124). (To satisfy Taylor's curiosity, my first two Bibles have been destroyed, but not for pious reasons. With multiplied millions of printed copies in circulation the situation and mentality now is completely different from what obtained over a thousand years ago.)

By way of a parting shot the reviewer informs us that "Mark 1:40 is cited as an example of conflation in Codex B (p. 60), whereas B actually omits the variant in question." Evidently he consulted only *UBSGNT*, which notices only one variant in this verse, "and kneeling to him." Now B does omit those words, but they are not "the variant in question." Does the reviewer not possess a Nestle text? He would have had to go no farther to discover the real variant in question—C, W and Theta read *kyrie*, Aleph and the majority read *hoti*, and B is alone in reading *kyrie hoti*.

I hope that the reader will sympathize with my disappointment at the superficiality that characterizes Taylor's review of my book. I am still waiting to see a review that really deals with the issues and evidence.

[3]K. Lake, R. P. Blake and S. New, "The Caesarean Text of the Gospel of Mark," *HTR* 21 (1928) 349.